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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modeling of 2008 Kasatochi volcanic sulfate direct radiative forcing: assimilation of OMI SO_2 plume height data and comparison with MODIS and CALIOP observations" *by* J. Wang et al.

Anonymous Referee #2

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General comments:

This work utilized the GEOS-CHEM CTM to simulate the transport and lifecycle of SO2 and sulfate aerosols from the 2008 Kasatochi eruption. SO2 plume height data from OMI were used to provide better initial conditions for model simulations. The author also used the MODIS and CALIOP retrievals to evaluate the model and calculated the radiative forcing of volcanic sulfate aerosols using a radiative transfer model. This is an interesting and well organized paper. The numerical experiments are also well designed. In addition, the authors provided a nice introduction of various satellite

retrievals and their potential applications in model evaluation and in providing model initial conditions. I would recommend publication of this manuscript once the authors addressed my comments below.

Specific comments:

Page 26442:, Line 8: How accurate is the MODIS cloud liquid water retrieval? A discussion about the data quality would be beneficial here. For example, Min et al. (2012, ACPD) reported that over Southeast Pacific the agreement between MODIS retrieval and in-situ measurement depends on the adiabatic status of the cloud.

Page 26443, Line 20: What's the time step used in the model? Is it the same as in the meteorological/re-analysis forcing data (3 hours)?

Page 26444, Line 27: "A good agreement with no bias was found . . ." I think the performance of the model is overstated here. Compared to the observation, models always have some bias in certain aspects (e.g., in specific regions or seasons).

Page 26445, Line 20: Why one day duration was assumed here? Is it a reasonable assumption?

Page 26446, Line 3-5: Based on the statement here, it seems that the hygroscopic growth is only considered for optical property calculation. However, (wet) particle size is important for sedimentation and turbulent dry deposition calculations. Using dry size for these calculation will underestimate the particle sedimentation rate. A discussion about this would be helpful.

Page 26446, Line 5-8: It seems to me the externally mixing was assumed in the calculation. Whether it is true or not, it is necessary to mention the mixing assumption in the calculation.

Page 26446, Line 11: "all-sky" or "total-sky" is more commonly used than "full-sky" in literature.

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Page 26446, Line 16-22: Is the geometric radius (0.07um) for dry aerosols or wet aerosols? If it is the dry radius (as I assume), using a fixed geometric radius of 0.19um to consider the enhancement of hygroscopic growth by sulfuric acid would not be appropriate. What's reasoning here?

Page 26448, Line 3: Fig. 2c, i, o

Page 26448, Line 6: C is not in Fig2m. Do you mean A?

Page 26448, Line 20: How about the variance?

Page 26451, Line 23-24: It would be helpful to plot the modeled tropopause height in the figure. Also, how do we know the backscattering is because of SULFATE aerosol loading? Would it be possible that the signal we see in Fig. 6a (A,B) is caused by cirrus cloud and other types of aerosols? How well can CALIOP lidar distinguish aerosol particles from ice particles in cirrus?

Page 26452, Line 2-5: C, D and G should be in Fig. 6ab. and Fig. 6cd. Also, I can't agree to the statement here. The observed vertical-integrated backscattering and modeled extinction in region C are much larger than those in regions A and B. How do we know the scattering/extinction was caused by deposition? As the authors mentioned later (Line 12, same page), the influence of non-volcanic aerosols (including aerosols other than sulfate) might dominate the signal below 10km in the CALIOP data. More evidence is needed to support this.

Page 26454, Line 3-8: A discussion about the differences of injection heights and circulation patterns (that is related to poleward transport) between the Pinatubo eruption and Kasatochi eruption would be helpful.

Page 26454, Line 18-21: The TOA forcing in GEOS-CHEM and surface forcing in Kravitz et al. (2012) are incomparable. Different models have different relationships between surface and TOA forcings. Is there surface forcing output in your model?

Minor comments:

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Page 26437, Line 11: an e-folding time . . .

Page 26437, Line 20,22, and many more: Please check the special character. I can't see them in Preview (Mac).

Page 26445, Line 21: set to 10km

Page 26451, Line 20: The blue line in Fig.5 is hard to see. Please change it to black is possible.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26435, 2012.